which have few parallels in the history of science. In the presence of Professor Baird's immense services to science in America, we can nevertheless pause to pay a tribute to the worth of his character. Perhaps his leading trait was a comprehensive benevolence which knew no distinctions, but embraced all in its benefactions to the limit of possibility.

Professor Baird's disinterested love of science will not be lost to us by his death. To his foresight is due the fact that he will probably be succeeded by men equally catholic with himself, and equally able to maintain the dignity of science at the national capital. We refer to Professor S. P. Langley, the present assistant secretary, and Mr. G. Brown Goode, assistant director of the National Museum.

GENERAL NOTES.

GEOLOGY AND PALÆONTOLOGY.

On the Morphology and Origin of the Ichthyopterygia.[1]—There is no group of reptiles, the Testudinata perhaps excluded, of which the morphology of the skeleton and the origin has been more discussed than that of the Ichthyopterygia.

As the name says, the Ichthyosaurs were long considered as animals which had retained characters of fishes. The limb-bones developed as paddles were regarded as original stages, forming a link between the fishes and the higher Vertebrata. I shall show in the following note that the Ichthyosaurs are specialized Sphenodon-like reptiles, and that their fins are not original but secondary formations, like the paddles of the Cetaceans.

The Skull.—The skull is only comparable with that of the Rhynchocephalia, especially Sphenodon and the Lacertilia. The only real difference is that, like in the Cetaceans, the anterior part of the skull has been very much elongated. The *general* structure of the skull resembles that of the dolphins. In its *morphology* it is a copy of the Sphenodon skull.

The foramen magnum is bounded by four bones,—the large basioccipital, the two exoccipitals, and the supraoccipital. These parts are well known. Of the otic bones two are separated; the third, the epiotic, if ever free, is coossified as in all reptiles with the supraoccipitals. The opisthotic is a pretty large conical bone, touching the exoccipital and supraoccipital.

[1] A paper read before the American Association for the Advancement of Science, August 12, 1887.

The prootic is a small flat elliptical bone, connected with the base of the skull, the supraoccipital, and the opisthotic. The structure of the bones shows that there must have been developed large masses of cartilage, still more than in young sea-turtles, connecting these bones.

Between the basioccipital and the quadrate a strong handle-like bone is expanded, fitting in a grove of the quadrate. This is the stapes, as Professor Cope correctly suggested, exactly in the same position as in Sphenodon, in which it is of similar shape but not so stout.

Turning now to the upper part of the skull, we find the parietal bones of exactly the same structure as in Sphenodon, and in front of those the very small frontals.

The parietal foramen, which is very large, as in Sphenodon, is situated between the frontals and parietals or in the frontals, in a similar way as in Sphenodon. In the upper part of the Sphenodon skull the parietal foramen is formed entirely by the parietals; in the lower part by both the parietals and frontals. The nasals in Ichthyosaurus are very large bones, touching the frontals, parietals, postfrontals, prefrontals, lachrymals, and premaxillaries.

We have now to consider that part of the skull situated between the supratemporal fossa, the orbits, and the quadrate.

The supratemporal fossa is formed by three bones,—the parietal, the postfrontal, and a big bone forming its posterior and exterior border. This bone, which is connected also with the quadrate and another bone which joins the quadrate, quadratojugal, postorbital, and postfrontal, is the *supratemporal* of the Lacertilia, which is united in Sphenodon to the squamosal.

This bone is called mastoid by Owen, squamosal by Seeley and Cope, but it is in fact the supratemporal bone of the Lacertilia.

Between this bone, the postfrontal, postorbital, quadratojugal, and quadrate, another bone is articulated. This bone represents the *true* squamosal of the Lacertilia, the *prosquamosal* of Owen, and the *supraquadrate* of Seeley.

The place which is taken in Ichthyosaurus by the supratemporal and squamosal is taken in Sphenodon by one bone only. This represents the supratemporosquamosal.

The quadratojugal of Ichthyosaurus is connected with the squamosal, postorbital, jugal, and quadrate exactly as in Sphenodon, where it ossifies later with the quadrate.

There is a foramen in Ichthyosaurus, between the quadrate and quadratojugal just as in Sphenodon. Ichthyosaurus has an epipterygoid (columella), as A. Smith Woodward has shown, of the same form and connection as Sphenodon.

The lachrymal is free from the prefrontal in Ichthyosaurus, as in many Lacertilians; it is united with that bone in Sphenodon.

The vertebræ of Ichthyosaurus are only specialized from a Sphenodon-like form in which the notochordal character was

already lost. The ribs are two-headed, and are always connected with the centrum of the vertebræ. There are only three small intercentra developed between the basioccipital and atlas, and the two next vertebræ.

The abdominal ribs consist of a median piece just as in Sphenodon, to this one or two lateral pieces are attached on both sides. In Sphenodon there are two pairs of abdominal ribs connected with one pair of true ribs, in Ichthyosaurus only one.

The shoulder-girdle is only comparable with that of Sphenodon; the clavicles and interclavicles are very much alike; the scapula and coracoid resemble very much the same elements in young Sphenodons when they are still free.

The pelvis of Ichthyosaurus is in a rudimentary condition; it contains the three elements, but a comparison with other pelves is of no value.

The limbs are specialized for the animal's life in water from some land-living form, as I shall show.

The limbs or paddles of the Ichthyopterygia were a principal reason for giving a peculiar position to those animals in the system.

I have shown before, on logical reasons, that the paddles of Ichthyosaurus were not original but adapted. To-day I can give the proofs for this.

In the Museo Civico of Milano, in Italy, are some specimens of Ichthyosaurus from the Trias of Besano. They have been noted lately by Professor F. Basani, and called *Ichthyosaurus cornalianus*. The specimens measure from 50 to 90 cm. only.

The teeth are of smaller number than in the other Ichthyosaurians, and of two forms. But the most important character not mentioned by Basani is the structure of the limbs. Radius and ulna are not as in the other Ichthyopterygia, short bones which touch each other without space, but they are more elongate, leaving a space between them. This one character alone is sufficient to establish a new genus, which I propose to call *Mixosaurus;* constituting a distinct family, the *Mixosauridæ*.

The fins of Baptanodon or Sauranodon Marsh have been regarded as the oldest ones. It is just the reverse of it; they are the newest, most specialized forms.

The bone considered by Professor Marsh as intermedium is not this element, but the ulna; the ulna of Professor Marsh represents probably the pisiforme.

The oldest Ichthyopterygia had few phalanges and not more than five digits; radius and ulna were longer than broad, and separated by a space. Later, through the adaptation to the water, more phalanges were developed, more digits appeared, mostly by division of the former or by new formation on the ulnar side. I have never found a new digit developed on the radial side.

The fin of Baptanodon was developed by the trans-location of the pisiforme or another element of a new-formed ray to the humerus.

Just the same we observe in the Sauropterygia.

In the oldest Triassic forms, the Lariosauridæ, we have forms with extremities, where radius and ulna are long bones, and the number of phalanges as in the Lacertilians. In the Liassic specimens, the Plesiosauridæ, radius and ulna become smaller, the number of phalanges enlarged.

In the specimens of the Kimmeridge, *Pliosaurus*, (? in all) we have three short bones connected with the humerus, just as in Baptanodon.

The Cetacean paddle has developed in the same way as Ryder has suggested; a new proof is given by the Sirenidæ.

Flower says, in the last edition of his "Osteology of the Mammalia," that the number of phalanges in the Sirenia is never increased in number beyond the limit usual in the Mammalia,—*that is, three.* But Dr. H. Gadow, in Cambridge, England, showed me a manus of *Manatus americ.*, prepared in alcohol, which contained a fourth small ossified phalange in the third digit, and one of *Halicore dugong* which contained an ossified fourth phalange at the fourth, and a cartilaginous fourth in the third digit.

This shows how the supernumerary phalanges were developed, and it is remarkable to say that the embryo of *Halicore* examined by Leboucq contained only three phalanges. The increase takes place during the life of the individual through mechanical influence.

I do not longer doubt that the Ichthyopterygia were developed from land-living reptiles which very much approached the Sphenodontidæ.

Classification of the Ichthyopterygia.—a. Radius and ulna elongated, separated by a space in the middle. Teeth of two forms, but not so numerous as in the Ichthyosauridæ. Small animals. Triassic.

Family *Mixosauridæ* Baur.

Genus *Mixosaurus* Baur.

b. Radius and ulna short bones, touching each other. Teeth well developed and numerous.

Family *Ichthyosauridæ* Bonaparte.

Genus *Ichthyosaurus* Koenig. There are, however, several genera contained in Ichthyosaurus, as Seeley has already suggested.

c. Radius, ulna, and a third bone articulating with the humerus; teeth rudimentary or absent.[1]

Family *Baptanodontidæ* Marsh.

Genus *Baptanodon* Marsh.—*Dr. G. Baur, New Haven, Conn.*

[1] I have seen small teeth at the end of the jaws of the Baptanodontidæ contained in the private collection of Mr. Leeds in Peterborough, England.

The Address of Vice-President G. K. Gilbert before Section E; A. A. A. S., Columbia College, New York, August 10, 1887.—Mr. Gilbert's address was a well-written composition, which bore the same title as the pamphlet published by the undersigned, under the auspices of the American Committee,—"The Work of the International Congress of Geologists." The easy and flowing style of the address was to have been expected by those who had read any of Mr. Gilbert's previous productions; but it must not be supposed that his fecundity of expression led him into saying too much. He has not been betrayed, for instance, into any general mention of the past work of the American Committee delegates to the Congress; nor even into any particular mention of that part of it which furnished the greater part of the material on which his address was based.

In speaking of the need for a word "to denote indefinitely an aggregate of strata," since to the word *formation*, formerly used in this manner, the Congress has attached a specific meaning, he says,—

"I suggest that we may advantageously enrich our language by the permanent adoption of *terrane*, a word whose English meaning has not been well established."

This is a singular, though doubtless inadvertent, plagiarism, since the two international committees (see "Report of Am. Com.," p. 43, etc., *a*, *c*, *d*, and pp. 50, 9, etc.) have employed the word in just this way. In the debates of the Congress also (*ibid.*, p. 22) "Professor Renevier proposed the term *terrane* (*terrain*) to avoid prejudging the rank in the classification of these rocks" (Archean). "He objected to the use of the term in any systematic sense, but believed it might be employed in a general sense." This, of course, inferred the use of the French word, with whatever pronunciation and spelling, in other tongues.

His suggestion as to the use of the termination "al" to convey ideas of time, as in Archeal, Cretaceal, Laramal, Belly Riveral, Bitter Creekal, if it will induce the local-taxonomy geologists of Mr. Gilbert's school to admit, through its employment, any generality in geological classification, will probably be easily introduced. Still, after having conceded to him an unusual command of his mother-tongue and great skill in its use, one cannot help regarding his innovations as usually more *logal* (as perhaps he himself would have phrased it of another) than logical. Nevertheless, let us hasten to observe that they are always euphonious; and, in fact, in reading his well-rounded periods, one may imagine them the agreeable cadence of word-drops caught up by some sprite of the river of language, and with the added glory of hue and tone from the intellectual light which floods them, falling back into their native bed.

In the serious matter of the address there are some passages to approve and some to disapprove. For example, in speaking

of the Congress's suggestions as to the terminations to be added to indicate groups, systems, series, etc., Mr. Gilbert says,—

"It would be impossible for a geologist to name or allude to a terrane without declaring its rank," etc.

To this it may be said that schemes of classification are not made for systematizing what we do not know, but what we do know. Nevertheless, it would not be an insuperable obstacle to speak of the "blankary, blankic, or blankian terrane." Equally ill-founded is the fear that by too systematic a classification—

". . . Taxonomy would be conceived by many geologists as an end instead of a means, . . . and energy would be wasted in taxonomic refinement and taxonomic controversy."

Energy is wasted now, and a great deal more than if there were some fixed principle to guide the controversialists.

One line of objection, curious in one who must be assumed to have accustomed himself to take broad views of things, is this:

"A committee appointed for the purpose formulated rules for the establishment of the names of genera and species, and their report was adopted by the Congress. I have no opinion to express as to the wisdom of the rules, but it is a matter of surprise that a body of geologists assumed to speak with authority on the subjects. From one point of view palæontology is a part of geology; from another point of view it is a part of biology. In so far as it names genera and species it is purely biologic, and it would seem proper that students of fossils unite with students of living animals and living plants in the adoption of rules of nomenclature."

Is it forgotten by Mr. Gilbert that the field of palæontology has been occupied almost exclusively by students of living plants and animals on account of its rich promise of results in determining those questions of derivative origin with which the address of this year's president of the Association was concerned? These geologic-biologists specially equipped to prosecute new inquiries are the very members of the Congress who have attempted the classification to which he alludes. He adds:

"The same is true of mineralogy, in regard to which no action has been taken. The most intimate relations of systematic mineralogy are with chemistry."

The last sentence would hardly secure Mr. Gilbert's critical approval were he aware that, from the earliest dawn of the science of mineralogy up to the present time, some of the most pre-eminent among mineralogists have denied this. But, even if it were so, some of those best able as chemists to grapple with petrological problems have entered the field of geology for the purpose of devoting themselves to it. Towards the close of the address he touches again upon this point, as follows:

"When a matter is proposed for regulation by the Congress, the first question which should be asked is whether it falls within the legitimate purview of a convention of geologists."

But what does not fall within such purview which bears, however indirectly, upon the riddles they are trying to unravel? Is the science of geology to be classed with those occupations

which relate solely and exclusively to one thing? (Perhaps Mr. Gilbert would call them *monotaxic*.) Or is it not rather a campaign requiring for its prosecution the infantry, artillery, cavalry, pontoon-trains, sappers and miners, engineers, and all the various departments of the largest army of science? If civil engineers with a little experience in camp become among the best military engineers, if our frontiersmen make the best scouts, why may not biologists, chemists, and crystallographers whose ambition urges them to seek a wider field than their laboratories become the very leaders which geology demands? In point of fact, they do; and if Mr. Gilbert will observe in the debates of the Congress, or of any other body which discusses these questions, who take the principal parts, and whose influence is most felt, he will find men eminent in the domains of special knowledge before they ventured to advise in that realm where it was applied to the attainment of a particular purpose.

It would seem, from certain parts of the address, that Mr. Gilbert half fears and half denies that the Congress possesses infallibility, but thinks that it should never pronounce in favor of a classification until all knowledge relating to the subject has been exhausted. He says, in regard to the acceptance by the Congress of a classification of the eruptive rocks,—

"With the rapid growth of knowledge and ideas, classifications are continually remodelled, and the best is in danger of becoming obsolete before it has been printed and circulated."

This evinces a curious view of the purpose of science. Is the same not equally true of philology, of ethnology, and of anything else which can be made the subject of investigation and transformed into organized knowledge? Does Mr. Gilbert expect that the heavens will fall if the classifications of the Congress need remodelling when time and further study have given more light? Are we to wait for omniscience before we attempt to classify? Or will not, rather, the changes which the future is certain to require, however great, be easier and less distracting when the whole enlightened world can take them at once by mutual agreement?

But perhaps Mr. Gilbert does not consider the co-ordination of knowledge in geology essential; one would judge so, at least, by his remark that

"—every system and every group is local. . . . If I have properly characterized stratigraphic systems, if they are both natural and local, it goes without saying that the classification of the strata of all countries in a dozen or so systems . . . is impossible."

Were that qualifying "if" not there one would be tempted to remark that, though the writer thinks it goes *without* saying, very few will concede that this statement goes *with* the saying, and in the absence of any proof. It is a thing which Mr. Gilbert cannot possibly know enough to deny, although he may think

that none of the supporters of the proposition know enough to assert it.

In parts of the address there is some appearance of unfairness in stating the position of the Congress. Take this, for instance:

"There can be no doubt that those who originally organized the work contemplated the *enactment* of a stratigraphic classification to be applied to the entire earth, and the selection of a color scheme for use either in all geologic maps or in all general geologic maps. But at the Berlin session the committee in charge of the work on the map of Europe pressed the Congress for the determination of questions on which hung the completion of the map, *and many hasty decisions were reached; while not a few disputed points were referred to the Map Committee.* The debates indicate that much *or* all of this work was provisional or of merely local application, but the *resolutions* adopted show little qualification. It should be added that the official minutes of the meeting are still unpublished."

—[It might, with grace, also have been here added whence his knowledge of the unofficial minutes came.]—The use of "enactment" in this sense conveys an impression the reverse of the fact. The Congress assembled to consult, to compare notes, and recommend; not to enact. Of course, it was, and is, hoped and believed that some plan will be arrived at of universal application. Without this, geology would be simply a more intellectual pastime than collecting postage-stamps or measuring property lines; but the implication of the entire succeeding sentence is, that the Congress, being pressed, "enacted" certain things, and left other things to be "enacted" by a subordinate committee. That all the points decided had reference to an experiment which was to be made the subject of future criticism is rendered unclear by the "much *or* all," and by the little qualification which it is said the resolutions have.

Yet this should have been understood by Mr. Gilbert, for he says, farther on,—

"By a series of resolutions a partial scheme has been selected, one color at a time, and the completion of the plan has been left to the Committee on the Map of Europe."

This certainly implies that the Map Committee has consulted the Congress as to certain portions of the former's task, and not that the latter has "enacted" colors for the world. Further:

"It is understood, in a general way, that the Congress reserves final action, and that the published legend not only belongs specifically to the map of Europe, but is provisional; still, as the map, *if generally approved*, will unquestionably be declared by the Congress an authoritative pattern for the guidance of map-makers, the plan should be freely criticised at its present stage."

If the map is open to much criticism it cannot be generally approved, and if not, it is quite proper that it should be declared a pattern. The Congress has quite abundantly shown its aversion to dictating or enacting contrary to the judgment of qualified objectors. Mr. Gilbert

. . . "cannot too strongly nor too earnestly insist that a System which is universal is artificial."

Of course, all human systems are artificial; but if Mr. Gilbert means inapplicable, and if any considerable number of competent geologists agree with him, it must be admitted that the very idea of an international congress is an absurdity. It may be doubted, however, whether this be the case, when one considers the many eminent men who have interested themselves in the Congress. Passing over his statement that no strata in the Eastern United States are pronounced Jurassic with confidence (as one in which he differs from at least as good an authority as himself), he objects to separating "a continuous rock system" (conformable strata?) which contains fossils resembling forms of the lower Jurassic at the top, and lower down others resembling forms of the Triassic, into those two systems, and says, "A Jurassic system thus established would be necessarily artificial." But in Great Britain the beds from the bottom of the Primordial to the base of the Upper Silurian make a single conformable sequence; and many other examples in Europe and this country might be cited. Indeed, it is strongly insisted by the Director of the United States Geological Survey that "systems or divisions of the second order (above the Archean) should be based primarily on biology and secondarily on physical structure." Mr. Gilbert's definition of a System as

"a great terrane separated from terranes above and below *by great unconformities*, or great life-breaks, or both,"

differs from that of his chief, as well as from the common usage.

But Mr. Gilbert prefers his

"own definition of a system, making it natural, and consequently local;"

and he opposes

"any attempt to coerce the geology of one country in a rigid matrix formed over and shaped by the geology of another country."

As to the latter, so do all geologists; but it is a different proposition to make a matrix by combining the "geologies" of all countries and fitting as much as there happens to be of each into the general mould, leaving the others to complete the gaps.

He objects to the provisional color scheme of the Map Committee, that

"—it is adjusted to the rock systems of Europe exclusively, and makes no provision whatever for the systems of other parts of the earth. The geologists of Wisconsin, for example, cannot use it without calling the Keweenawan either Cambrian or Archean," . . . if they believe that "it belongs to neither of these two categories."

The above, to be intelligible, needs a note explaining that certain eminent geologists have lately come to the conclusion that the term Archean should be restricted to the lower part of what up to the present time the world has understood it to define, and that another term should be applied to the rocks between its upper portion, as thus defined, and the base of the Primordial.

This criticism, then, assumes the form of a serious sort of pun, or a puzzle based upon the ambiguity of a word. As the greater number of the world's geologists have not yet accepted this new use of "Archean," the criticism is not fatal. Of course, if the new group be accepted, the Keweenawan will be interpolated between it and the Paleozoic.

It is not the purpose of this article to criticise the plan of the spectrum proposed by Mr. Gilbert. As he says, it has been very fully discussed in the proceedings of the Bologna Congress; but it may be said that one of his canons—viz., that the groups of "hues and tones," called by the vulgar simply colors, which are to serve as a scale—

"... must be so chosen that the degree of separateness of adjacent colors shall be everywhere the same, as judged by the *normal* human eye."

will be very difficult to carry into effect, in default of the discovery of, and agreement upon, that normal eye. To no two eyes of the ordinary kind would the "separateness" be likely to appear the same. It ought also to be mentioned that, in adding the hues of purple to one end or the other of the spectrum to increase the range of the time-scale colors, he violates his own rule, and renders it impracticable to assign by its wave-length a color to an intermediate rock-mass.

The portion of the address, however, most open to objection is the end of it. He says,—

"A classification is a generalized expression of the facts of observation outside the domain of the voter. If it comprises all the essential facts its sufficiency will be eventually recognized, whether its authority is individual or collective. If it does not comprise them it will inevitably be superseded, by whatever authority it may have been instituted."

This argument savors somewhat of Oriental fatalism, but no exception need be taken to the words beyond the fact that a vote of competent men is a good way to arrive at a conclusion whether a certain formulative statement be or be not the desired generalized expression. The conviction that such a vote carries will be strong in proportion to its unanimity, but, in any case, it will have to stand the test to which Mr. Gilbert alludes. He proceeds:

"For this reason I am opposed to the classification by the Congress of the sedimentary formations, and likewise to the classification of the volcanic rocks; and I also regard it as ill advised that the Congress undertook the preparation of a map of Europe, for that, if more than a work of compilation, is a work of classification."

Poor Congress! About to be bereft of the power to consider the objects for which it was created, why was it called into purposeless being? Suppose that when Lavoisier and his coadjutors assembled the famous congress to decide upon a system of nomenclature and to agree upon a theory of chemistry (then largely through his efforts emerging from chaos to order) some one had said, "Your congress should consider no questions of science,

but only questions of names. Within the field of names it should only comprehend what Stahl and the phlogistonists understand by them. It should therefore not attempt the classification of the elements, nor the classification of compounds made by combining elements. It should discuss no analysis unless the data be compiled from existing authorities. It should not regulate the nomenclature of somatology, for that belongs to physics and mechanics; it should not regulate the nomenclature of reactions, for that belongs to magic,—in fact, it should not meddle with names at all, for that is the domain of philology:"—would we ever have had a science of chemistry at all, or would Lavoisier and his colleagues have persisted in their reckless course of counselling and devising?

In this connection the words of the great Berzelius are very applicable:

"In every science a systematic nomenclature is necessary, but in none more than in chemistry. The confusion which reigned prior to the happy idea of Guyton de Morveau is a proof. The nomenclature which chemists have employed since 1780 is the fruit of his labors, sustained and directed by Lavoisier, Berthollet, and Fourcroy. The advantage that it presents is that whoever learns to recognize a compound can give it its true name without knowing it beforehand, so that it is unnecessary to charge the memory with a large number of different terms. . . . Furthermore, scientific nomenclature is in itself the expression of a complete theory, so that if, on the one hand, the theory furnish the name, on the other the name indicates the theory. It has been objected to this relation of a nomenclature to theory that it obliges the names to be changed with the theories, which would not be necessary in the employment of purely technical terms, that are always preserved without alteration. But as these changes are ordinarily the results of progress in the direction of clearer notions, the change of nomenclature, far from injuring, is, on the contrary, another means for facilitating the march of ideas. *In general, nothing which contributes towards rendering any part of a science stationary is advantageous:* everything must advance equally in proportion to the multiplication of discoveries and knowledge." (Berzelius, "Treatise on Chemistry," vol. i., Introduction, 1829.)

It is tolerably sure that if Mr. Gilbert expects the International Congress of Geologists to eschew all questions of science in its debates, and devote itself to the employment of playing at logograms exclusively with old data, he will be disappointed.—*Persifor Frazer.*

On the Homologies of Edestus.[1]—The genus Edestus was formed by Professor Leidy to comprise certain singular fossils from the coal-measures of the West, about which various theories have been held. The one generally received is that they are dorsal spines used as weapons of defence or offence. About a year and a half ago Dr. H. Woodward, of London, suggested that they might be pectoral fins similar to those of Pelecopterus, a genus described by Professor Cope from the Cretaceous of Kansas. The absence of any trace of articulation, the apparent evidence that there were ligaments extending to the base of

[1] Abstract of a paper read before the American Association for the Advancement of Science, August 15, 1887.

the denticles, the segmented character of the fossils, and the fact that the segments are found singly, and of different sizes, which would indicate that they are shed like teeth, together with the presence of a slender curved basal bone (similar to that in Onychodus of the Devonian of Ohio) in the Australian form described by Dr. Woodward, led the author to think that these fossils are neither dorsal spines nor pectoral fins, but intermandibular teeth, which had a membranous or cartilaginous support in the American forms, and an osseous support in the Australian. These supports would bear the same relation to the mandibular arch that the glosso-hyal does to the hyoid arch.—*Fanny R. M. Hitchcock.*

MINERALOGY AND PETROGRAPHY.[1]

Petrographical News.—In the continuation[2] of his work on the rocks collected by the "Challenger" expedition, Renard has reached the discussion of the geology of the islands in the Indian Ocean. Those situated in the meridional part of the ocean are divided into five groups, three of which have been examined in detail. They are all of volcanic origin, and are in no way connected with Madagascar or the land within the Antarctic circle. The island *Marion*[3] is formed of volcanic rocks of two distinct ages, readily distinguishable by differences in the character of the vegetation they support. They both consist of feldspathic basalts, with anorthite as the plagioclastic constituent. The rocks of the island *Heard* are very similar to those of *Marion*. In the neighborhood of Corinthian Bay the prevailing rock is a feldspathic basalt, in which Baveno twins of bytownite are crowded together in groups. Many of these are optically anomalous in their action between crossed nicols, a fact supposed by Renard to be due to their fine lamellation. The olivine shows cleavages parallel to oP and a pinacoid. In some cases it has suffered alteration into pilite. In addition to the basalts a few specimens of limburgite were also collected.——The same author has recently studied the rocks of Kerquellen Land,[4] which had already been examined to some extent by J. Roth.[5] Renard finds that these rocks consist principally of basalts, with amygdules of analcite and zeolites. The grains of olivine in them are grouped together like the chondra of meteoric stones. In addition to the basalts there occur on the island trachites, limburgite, and phonolites. Of these the trachytes and phonolites are older than the basalts.——Two late articles on the petrography of the Tyrol add several interesting facts to our knowledge of this re-

[1] Edited by Dr. W. S. BAYLEY, Madison, Wisconsin.
[2] American Naturalist, Notes, 1886, p. 640.
[3] Bull. d. l. Soc. Roy. de Belg., 1886, iii. p. 245.
[4] Bul. du Mus. Roy. d'Hist. Nat. de Belg., iv. p. 223.
[5] Monatsb. d. Kön. Akad. Berlin, 1875, p. 723.